

SCIENCE

FRIDAY, JULY 28, 1911

CONTENTS

<i>Mechanisms of Cell Activity</i> : PROFESSOR CARL L. ALSBERG	97
<i>The Total Solar Eclipse of April 28, 1911</i> : DR. L. A. BAUER	105
<i>Samuel Calvin</i> : PROFESSOR GEORGE F. KAY .	106
<i>Scientific Notes and News</i>	107
<i>University and Educational News</i>	112
<i>Discussion and Correspondence</i> :—	
On "Soma Influence" in Ovarian Transplantation: PROFESSOR W. E. CASTLE. Genotypes are the Species upon which Genera are based: DR. CHARLES H. T. TOWNSEND. Measuring the Merit of English Writing: DR. W. H. DALL. Latin Diagnosis of Fossil Plants: DR. EDWARD W. BERRY. Executive Responsibility: PROFESSOR SIDNEY GUNN. Academic and Industrial Efficiency: PROFESSOR WM. G. RAYMOND. The Methods of a Veteran Investigator and Teacher: PROFESSOR BURT G. WILDER	113
<i>Quotations</i> :—	
The Department of Agriculture and Dr. Wiley	121
<i>Scientific Books</i> :—	
Contributions to Medical Science of Howard Taylor Ricketts: J. E. Howe's The Geology of Building Stones: DR. GEO. P. MERRILL. Tutton's Crystallography, Groth's Optical Properties of Crystals: PROFESSOR CHARLES PALACHE	122
<i>Special Articles</i> :—	
West Elizabeth, Pennsylvania, Deep Well: PROFESSOR THOMAS L. WATSON. Note on Reticulated Fish-scales: PROFESSOR T. B. A. COCKERELL. The Genus Typha and its Nematode Root Gall: L. N. HAWKINS. Correlation Notes: ALBERT B. REAGAN ...	125
<i>Societies and Academies</i> :—	
The Torrey Botanical Club: B. O. DODGE .	128

MSS. intended for publication and books, etc., intended for review should be sent to the Editor of SCIENCE, Garrison-on-Hudson, N. Y.

MECHANISMS OF CELL ACTIVITY¹

EVERY scientist who concerns himself with the problems of his own specialty must devise for himself certain concrete pictures of the nature of the fundamental units with which his specialty deals, in order that he may have a concrete form in which to clothe his thoughts. Thus each chemist must form for himself some sort of concrete notion concerning fundamentals, like atoms, molecules, chemical affinity, valence and ionization, imagery which he must avoid mistaking for absolute reality, and which he must be ever ready to shift and change and modify, in accordance with the development of chemistry.

The biochemist also has his imagery, only he takes the data of the chemist and physicist as the material out of which he constructs an imagery of his own, dealing not with atoms or molecules as such, but with conceptions of the physical and chemical nature of protoplasm.

I would present to you to-day the hypothesis which some biochemists have developed for themselves concerning the structure of protoplasm and the cell. Such a presentation must be very largely a personal one, for two biochemists would hardly be likely to agree on all the details, however much they might be in accord on the essentials. Consequently, what I am about to offer will contain nothing essentially new.²

¹ Address presented before the general meeting of the American Chemical Society at Minneapolis, Minn., December 28, 1910. Published by permission of the Secretary of Agriculture.

² For an earlier presentation cf. Hofmeister, Fr., "Die Chemische Organisation der Zelle," Braunschweig, 1901.

My excuse for offering it at all is that it may present views novel to an audience such as this; views that may be of interest because they deal with some of the phenomena of life.

The fundamental unit of the biologist was for many years the cell. The conception of the cell is really an anatomical one, and a mere anatomical consideration of it does not lead to an understanding of its functions. I can illustrate what I mean by discussing the ordinary gravity cell or battery. The anatomist would describe such a cell as consisting of a vessel of some sort of material containing a blue liquid in which there were a whitish mass and a reddish mass at different levels, each connected with a reddish strand projecting upward. Such an analysis is obviously imperfect because it would never lead us to discover that the cell is capable of producing an electric current. However, he who studies the current produced by such a cell, its external and internal resistance, and the like, is, in respect to the gravity cell, a physiologist. Such a physiologist gives us some insight into the functioning of the cell, but he tells us nothing concerning the cause of the generation of the current. The analytical chemist, on the other hand, would tell us that the gravity cell consists of such and such a percentage of silicates, zinc, copper, sulphuric acid and moisture, information very useful in its way, but no more so in the comprehension of cell activity than that furnished by the anatomist and the physiologist. To understand the gravity cell all three kinds of knowledge are essential. Structure counts quite as much as chemical composition. In such instances the chemist needs anatomical knowledge, for with chemical reagents alone one can not recognize structure. Without a knowledge of structure it is often quite as impossible to understand mecha-

isms as it would be to predict the regular movements of a watch by first smashing it and then determining by analysis that it contains certain percentages of gold, copper, carbon and tin *et al.*³ Watches may be made of many kinds of material and yet keep the same time. However important the material, it is the structure that is even more essential in measuring time. Apparently this seems equally true of living organisms. The energy moving a watch may be furnished by a spring of brass, rather than of steel. Similarly most living things obtain their energy by the oxidation of carbohydrate, but by no means all. For example, certain kinds of microorganisms, the Beggiatoaceæ, obtain their energy by the oxidation of sulphuretted hydrogen to sulphuric acid. They absorb sulphuretted hydrogen and excrete sulphuric acid. Most living things store up, as reserve sources of energy, some form of carbohydrate, such as starch or glycogen. Not so the Beggiatoaceæ; they store up elemental sulphur. The conclusion is therefore inevitable that for a proper understanding of the mechanisms of the living cell both structural and chemical knowledge is necessary.

Evidently the earlier notions concerning protoplasm were unsatisfactory because they were either purely anatomical or purely chemical, according to the bias of the investigator. Originally protoplasm was regarded as a material showing some anatomical structure, to be sure, but chemically more or less homogeneous, though very complex. The very term "protoplasm," the first formed, is nothing more than a definition of this conception. Later, as biochemistry advanced, protoplasm came

³ Similar opinions have been expressed, among others, by E. H. Starling ("The Mercers' Company Lecture on the Fluids of the Body," London, 1909).

to be regarded as a very complex though rather homogeneous mixture of materials, some of which were assumed to be alive. The latter were supposed to be huge, complex molecules, protein in the main, but not necessarily entirely so. The vital qualities inherent in protoplasm were supposed to run parallel with the complexity and instability of these huge living molecules. Essentially this was a chemical conception of protoplasm different from ordinary chemical conceptions only in that these hypothetical, hopelessly complex molecules were assumed in some way to possess the power of regenerating themselves when in consequence of their instability they fell to pieces. As we look back now, we can see that apparently the only reason why proteins were chosen to fill this rôle was that at that time far less was known about proteins than about the other constituents of cells. By some it was even imagined that if only it were possible for organic chemists to know the structure of such molecules completely, a deep insight would be gained into the causes of life. Others, less sanguine, believed that this could never be, because in the process of analysis the molecule ceased to live, was changed, depolymerized, so that what was studied had in the very process of study lost its vital properties. Even as recently as the beginning of Emil Fischer's work upon protein the hope was expressed that since he had brought the synthesis of protein within striking distance, a more intimate knowledge of the nature of life would follow. And yet, though it is now possible to make substances very similar to some proteins with very high molecular weights, and though we may rest assured that all the complex substances occurring in living things will eventually be so well known that they will be ranged without wonder and without comment with the vast horde

of organic compounds, nevertheless, by such achievements alone, only a limited insight into the mechanisms of life can be gained. The reason is that a study merely of chemical constitution, however necessary, will carry us but a very little way in understanding even the simplest processes which take place in protoplasm, unless it be combined with a study of structure, and of the dynamics resulting from both.

Now when I speak of structure I do not mean necessarily anatomical structure that is visible with the microscope. Still a study of even microscopic structure has been and is of incalculable value. With such studies, the name of Bütschli is indissolubly connected. He it was who first emphasized the fact that protoplasm has the structure of a foam or a network with interstices filled with material of a physical nature different from the network, or finally, the structure of an emulsion. Indeed, his anatomical observations led him to study foams, and emulsions, experimentally, in the hope of being able to interpret his anatomical studies more rationally. His experiments became of fundamental importance in stimulating work in certain fields of pure colloidal chemistry and of the dynamics of surfaces. He understood better than any one before his time the anatomical structure of protoplasm. He was the first to point out that protoplasm is heterogeneous, consisting of at least two phases touching each other by minimal or capillary surfaces.⁴ We shall learn to appreciate the great significance of this idea. Even Bütschli, however, found that many heterogeneous systems appeared homogeneous under the highest powers of the microscope, because the differences in refraction between the phases was insufficient to render them visi-

⁴O. Bütschli, "Untersuchungen ueber mikroskopische Schäume und das Protoplasma," Leipzig, 1892.

ble.⁵ Visibility depends upon size, differences in refraction, adsorption of stains or reagents and similar more or less accidental phenomena. Plainly there may be physical and chemical structure which does not happen to be visible. It is to such structure dependent upon the heterogeneous nature of protoplasm and to the consequences arising from its nature that I wish to draw your attention to-day.

Protoplasm must have some such organization, other than a merely chemical one. Otherwise we can not understand how so many intricate reactions can take place in an orderly fashion within the narrow confines of a single microscopic cell. Let me illustrate by an example. The yeast cell converts sugar into alcohol, carbonic acid and water. Under certain conditions it also converts sugar into glycogen which it may store for a long time within itself, or which it may soon reconvert into sugar and then into alcohol. Under certain conditions it may oxidize alcohol. It synthesizes protein and cellulose. It forms glycerine, succinic acid and amyl alcohol. It may reduce sulphur to sulphuretted hydrogen. It performs undoubtedly a whole series of cleavages, syntheses, oxidations and reductions, and yet, examined under the microscope, it appears fairly homogeneous. No structure is visible capable of explaining how in this small space so many processes can go on side by side in an orderly fashion without interfering with one another. In a single test-tube it is manifestly impossible. In any homogeneous medium it is manifestly impossible. However, in a heterogeneous medium, such as an emulsion, it is conceivable. We have merely to imagine the reactions as taking place in different phases and to remember that at the points of contact of two phases membranes

form. By the term "membrane" we must understand in this connection merely the condensation of material at a surface serving to separate two phases. We have then imagined a structure for protoplasm, part chemical, part physical, sometimes visible, sometimes invisible, in which many reactions might go on side by side as thoroughly separated as though in separate test-tubes. They would, however, go on more effectively than in separate test-tubes, because in these no influence of one reaction upon the other is possible, whereas in a heterogeneous system such an influence between two separated reactions is conceivable. A reaction taking place in one phase, may, either by yielding products soluble in another phase, or by changing concentrations, affect one or two or every other phase. Interrelations of this kind might very well lead to what is ordinarily termed coordination.

Indeed, there is some evidence that some of the coordination is of this type. When a piece of protoplasm dies, not all the functions which up to that time it has exercised, cease at once. Some of them may continue for a long time, and these are usually due to enzyme action. In the dying mass respiration may continue as well as many other functions, but while they may be qualitatively the same, quantitatively they differ. The balance present during life is destroyed and certain reactions gain the upper hand, eventually dominating the field till everything else is suppressed. Coordination, the great characteristic of life, disappears. Anything which intermixes protoplasm, hence disturbs the phases, destroys coordination. Freezing is one of the agents exerting this effect upon many forms of protoplasm, for by causing some of the water to crystallize, it most effectively disturbs the balance of the phases. We note this, for instance, in the

⁵ Ostwald, Wo., "Grundriss der Kolloidchemie," S. 32, Dresden, 1909.

freezing of potatoes. Potatoes contain a mechanism for converting starch into sugar. Ordinarily this mechanism is controlled by and coordinated with other mechanisms. Freezing disturbs this balance. The mechanism for the hydrolysis of starch runs riot and the potato becomes sweet. Another instance is the coloration of the leaves in the autumn. Leaves contain among other things chlorophyll, chromogens and an oxidation mechanism. Under ordinary conditions this mechanism is held in check. Frost, however, plasmolyzes the cells, intermixes the protoplasm, and the oxidations run riot. Chlorophyll is bleached so that yellow pigments, such as carotin and xanthophyll, which are ordinarily masked by the green, become evident, while other substances, either pre-existent or formed during plasmolysis, are converted into the brilliant pigments of our autumn woods.

Now ever since the processes of life have been studied at all chemically, the ease and efficiency with which protoplasm brings about reactions has filled the chemist with awe. Reactions which he is able to perform *in vitro* only with difficulty by the use of powerful agents and high temperatures, protoplasm brings about perfectly at low temperatures. If we pursue our conception of protoplasm as a heterogeneous system further we can imagine mechanisms by which such action is conceivable.

Many reactions go on rapidly in protoplasm to a high degree of completeness, while *in vitro* the rate may be slow and the yield insignificant. This becomes intelligible in a heterogeneous medium without necessarily resorting each time to the action of an enzyme. We have only to assume that the reaction takes place in one phase and that one or more of the products disappear into another phase as fast as

formed. In this way reactions might go on at a rate and to an equilibrium quite different from those *in vitro*, for the products of the reaction would be removed from the seat of reaction almost as fast as formed. Such a mechanism might be merely the result of different solubility in the phases.

A similar line of thought throws light upon the fact that a substance present in protoplasm as a whole, in such minute concentration that *a priori* its influence ought to be negligible, may nevertheless exert a profound effect. We need not necessarily assume that this apparently disproportionate effect is produced by a catalytic mechanism. In a heterogeneous system the concentration of a given constituent as determined by quantitative analysis of the whole may be a spurious value, because this constituent may not be evenly distributed throughout the heterogeneous system. It may be almost absent in one phase and concentrated nearly entirely in another, and this difference in distribution may also be merely the result of difference in solubilities. Hence in protoplasm substances may be concentrated greatly in definite localities and thus make reactions possible which at low concentrations would be infinitely slow.

An uneven distribution of the various constituents of protoplasm such as this, is not the only way in which the concentration may vary according to this multiple phase hypothesis of protoplasmic structure. Even in the same phase the concentration of the substances in that phase may be caused to vary greatly by the energy at work in surfaces. It is a well-known fact that substances which diminish surface tension or the tendency inherent in liquids to assume that shape which reduces the surface to a minimum, accumulate at the surface so that they are present in the thin

surface layer in greater concentration than in the interior. Now in such a system as we have imagined protoplasm to be, the sum of the areas of the surfaces of all the phases must be very great, and consequently the concentration of the various substances distributed through it must vary greatly, not merely according as they are distributed in one or the other phase, but also as they are concentrated still more at the surfaces of one or the other of the phases. This concentration at surfaces may be very great so that the substances are present as though under great pressure, and we must imagine that reactions are facilitated at surfaces just as reactions with gases are facilitated by high pressures. Finally, we can imagine reactions facilitated whenever conditions arise which diminish surface energy, for in that case free energy is produced in the narrow concentrated surface film. How this might facilitate reactions we can only conjecture.

So far we have considered only the grosser phases. Finer phases, however, exist in protoplasm. It is now pretty well established that colloidal solutions are multiple-phase systems. Cells contain colloids in solution, and these colloids usually are of the type known as emulsion colloids, which means that all the phases of the colloidal solution are fluid. We have then here phases of very minute dimension. Now it is well known that the energy acting at surfaces increases relatively with the increase in curvature of the surface. The curvature of these minute colloidal phases is very great, and the energy which acts in these infinitely curved surfaces is correspondingly powerful. It is quite possible that many of the phenomena which are termed enzymotic are brought about by forces of this type, for it is very probable that the class of agents termed enzymes is not a uniform one, but includes

many classes of quite different agents acting by as many different mechanisms.

We have seen how considerations of structure have led to conceptions of cell dynamics. Conversely, a consideration of these dynamics can lead us back to a deeper understanding of structure.

Protoplasm ordinarily contains 80 per cent., and upwards, of water. Some beings may contain even more. For example, the medusæ or jelly-fish, fairly firm structures though they are, contain but 3.7–4.6 per cent. of solids.* Of these solids over 3 per cent. are the sea salts, so that the bell of the medusa, as solid as a firm jelly, can almost be said to consist of organized seawater. Ordinary protoplasm is not as thin as this. Still of its 15 or 20 per cent. of solids a considerable portion is inorganic salts and other electrolytes, for the greater part in solution, so that they hardly produce solidity. The remaining substances consist of fats, proteins, lipoids and other colloidal material. Built of such materials, it is hard to see how an organism can have so nearly solid or rather semi-solid a structure as protoplasm. If, however, we imagine the materials of which protoplasm is composed as distributed in different phases, the difficulties are not so great. If we imagine the fat and the lipoid as present in a different phase from the water, being present as an emulsion, perhaps rendered permanent by some such substance as soap, and if we think of other substances such as the proteins present in a colloidal and viscous state, and, if we imagine both the crystalloids and the colloids distributed between the various phases, we can get a structure which will be as firm as protoplasm is known to be. Thus it is easy to take egg-albumen, oil and sugar solution and mix them so thoroughly that the re-

* Krukenberg, "Ueber den Wassergehalt der Medusen," *Zool. Anzeiger*, 1880, S. 306.

sulting emulsion is firm enough to handle with a shovel. A firm system of this sort is used technically as a lubricant. By the emulsification of certain heavy oils with less than one per cent. of water an emulsion so solid may be formed from the two liquids that it may be whittled with a knife.⁷ These considerations, however, do not tell the whole story, at any rate for plants. A part of the firmness of many plant structures is due to the phenomenon termed by plant physiologists "turgor." This phenomenon has much similarity to the inflation of a flabby, hollow, elastic balloon with gas. In turgor, however, the inflation is by water, not gas, and the inflating force is not mechanical but osmotic. How far turgor is responsible for rigidity in animal structures is not yet clear. Certainly something very similar exists in the red blood corpuscles.

We have been considering the concentration of substances upon surfaces of finely subdivided phases; but in obedience to the same laws concentration takes place upon larger surfaces. This phenomenon must occur not merely at the contact surfaces between the phases in the interior, but also upon the outer surface of the protoplasmic mass itself. In such places we must have a concentration of material. Indeed, it can be shown experimentally that many solutions form really quite firm membranes even when there is no chance for evaporation to take place.⁸ Very probably

⁷ Ostwald, Wo., *op. cit.*, p. 105.

⁸ Ramsden, W., "Abscheidung fester Körper in den Oberflächenschichten von Lösungen und 'Suspensionen'" (Beobachtungen über Oberflächenhäutchen, Emulsionen und mechanische Koagulation), *Zeitschrift für physikalische Chemie*, Bd. 47, S. 336.

Metcalf, M. V., "Ueber feste Peptonhäutchen auf einer Wasseroberfläche und die Ursache ihrer Entstehung," *ibid.*, Bd. 52, S. 1.

Zangger, H., "Die Immunitäts-Reaktionen als physikalisches spez. als Colloid-Phänomen," *Vier-*

this phenomenon is responsible in many instances for the formation of biological membranes and may also account for the differentiation of the external layer so frequently seen in cells. This might merely be the result of the concentration of material in the outside layer in consequence of surface action.⁹ Considerable evidence for the participation of surface forces in cell-membrane formation may be found in the studies on hemolysis, by which is meant the leaking of hæmoglobin through the membranes of blood corpuscles. Many of the substances which cause the cell membranes of the red blood corpuscles to lose their semi-permeability in this way, have a great influence on surface tension. Such are soaps and saponine. One of the actions of certain snake venoms is dependent upon the presence in the venom of a substance of this type.¹⁰

If this hypothesis of concentration of material at the cell surface be correct, then it is easy to understand how many cells have the power of regenerating a new membrane on a wound surface, such as is formed when an amœba is cut in two. The surface energies must begin at once to act at the new surface until it, too, has been brought into equilibrium with the interior just like the rest of the cell surface.¹¹

This hypothesis of membrane formation can not be applied, for the present, at any rate, to many specialized membranes such

teljahreschrift der Naturforschenden Gesellschaft in Zürich, Jahrgang 35, S. 441.

⁹ Overton, E., "Ueber den Mechanismus der Resorption und der Sekretion in W. Nagel's 'Handbuch der Physiologie des Menschen,'" Band II., Teil II., Seite 805-6.

Pfeffer, W., "Osmotische Untersuchungen, Studien zur Zellmechanik," S. 124, Leipzig, 1877.

¹⁰ Zangger, H., "Ueber Membranen und Membranfunktionen," *Ergebnisse der Physiologie*, Bd. 7, S. 138, 1908.

¹¹ Cf. Overton, *op. cit.*; Pfeffer, *op. cit.*, and Zangger, *op. cit.*

as are found in many plant cells and in the red blood corpuscles. Still surface forces may be concerned even in these morphologically distinct and permanent structures. Now colloidal substances are among those which tend to accumulate at surfaces. Many of them are easily coagulated, and changed in such a way that they become more or less permanently insoluble. Such colloids are said to be irreversible, because after having been put out of solution, they can not easily be made to revert into solution again. Irreversible colloids when they become concentrated at a surface tend to become coagulated. In this way membranes of a high degree of tensile strength and permanency may be made experimentally.¹² Something of the sort might very well occur in cells with a morphologically distinct cell wall.

Of the composition of cell membranes we have in the last decade achieved certain definite notions. The cell membrane is usually semi-permeable, only allowing certain substances to penetrate into the cell. Now there are a number of ways in which the semi-permeability of a membrane may be explained. One is that the penetrating substance dissolves in the membrane. Another is that it combines loosely with it. So if we note what substances penetrate into cells and what the solubilities of these substances are, we may be able to reach certain conclusions concerning the nature of the cell-membrane. This has been done and it has been shown that many of the substances which enter cells are very much more readily soluble in fats and lipoids than in water. Indeed, the narcotic effect upon cells of many indifferent substances is proportionate to their partition coefficient between water and oil. For apparently the same reasons free alkaloids which are soluble in oil seem to penetrate cells,

¹² Metcalf, M. V., *op. cit.*

whereas their salts only do so in so far as they are dissociated. For similar reasons undoubtedly the toxicity of certain metallic salts, such as the chlorides of copper and of mercury, is due in part to the fact that, being soluble in organic solvents, they enter cells rapidly.

The objection has been raised to the hypothesis of the lipid nature of the cell wall that it does not explain how certain substances like sugar, protein and inorganic salts which are all undoubtedly utilized by the cell in one form or another enter the cell. It has, therefore, been suggested that the cell wall has a mosaic structure, other material besides lipoids entering into its composition.¹³ This is very probably true, for if we are right in assuming that forces acting at surfaces take part in the formation of the cell wall, then all those substances which are present in the cell, and which have the property of diminishing the surface tension of water, will affect one another in regard to their concentration at surfaces. It is the same phenomenon that has been so much studied in the influence of one substance upon another in respect to adsorption upon solid surfaces.¹⁴ How different substances may influence the concentration of one another on surfaces such as those of cells we can as yet only conjecture, but it is entirely possible that the result may be a mosaic structure of the membrane. If this suggestion prove true, it is possible that protein takes part in the structure. It may be then responsible for the entrance in small amounts into cells of certain substances as required by the metabolism. Proteins, in their capacity as amphoteric electrolytes

¹³ Cf. Macallum, A. B., "Cellular Osmosis and Heredity," *Transactions of the Royal Society of Canada*, 3d Ser., Vol. 2, pp. 152 et seq., 1908.

¹⁴ Michaelis, L., "Dynamik der Oberflächen," S. 25, Dresden, 1909.

combine with salts.¹⁵ Indeed it is virtually impossible to prepare protein free from ash.¹⁶ It may be that salts enter protoplasm by combining with protein in the membrane. Even if this mechanism prove ultimately not to exist, all the possibilities are not exhausted. The lipoids, kephalin and lecithin, occur in combination with potassium and sodium.¹⁷ These compounds are freely soluble in anhydrous ether. The metal is not completely masked, but can become to a slight degree dissociated. Perhaps it is by forming such compounds that metals enter cells.

I hope I have shown that by the methods of the organic chemist alone we can not hope to achieve much insight into the mechanisms of protoplasm. These mechanisms are dependent upon structure and this organic chemistry is not capable of revealing. The mechanisms are themselves interrelated and coordinated. These relations and coordinations are not capable of study by the usual analytical methods. The process of analysis destroys them as it destroys life itself of which they are the most characteristic manifestations. These characteristics of life can be approached only from the basis of structure of some sort. For a proper understanding of it, anatomical, chemical and physical knowledge must be combined. The resultant alone offers the hope of widening our knowledge of the mechanisms of cell activity.

CARL L. ALSBERG

¹⁵ Cf. Mathews, A. P., "A Contribution to the General Principles of the Pharmacodynamics of Salts and Drugs," "Biological Studies of the Pupils of W. T. Sedgwick," pp. 103-4, Boston, 1906.

¹⁶ Harnack's ashless protein is really a protein with volatile ash—HCl. *Berichte der deutschen Chemischen Gesellschaft*, Bd. 23, S. 3745, 1890.

¹⁷ Koch, W., and Pike, F. H., "The Relation of the Phosphatids to the Sodium and Potassium of the Neuron," *The Journal of Pharmacology and Experimental Therapeutics*, Vol. 2, p. 245.

THE TOTAL SOLAR ECLIPSE OF APRIL 28, 1911

[PRELIMINARY COMMUNICATION]

ON the way to meet the *Carnegie* at Colombo, Ceylon, I was so fortunate as to make immediate connection at Suva, Fiji, for Apia, Samoa, by means of a small steamer, the *Dorrigo*, chartered by the German government to carry the mail. I journeyed next to Pago Pago, Tutuila, 80 miles distant from Apia, chartering a 30-ton motor boat and arriving at Pago Pago on Monday, April 24. Laying my plans before his excellency, the governor of Tutuila, Samoa, he very courteously put at my disposal the U. S. cruiser, the *Annapolis*, and furthermore gave me the assistance of some of his best officers and men.

When I left Washington on March 16 the possibility of getting into the belt of totality in time seemed too small to warrant taking with me skilled assistants or elaborate outfits for chance eclipse observations. However, I took two magnetometers and Mr. Abbott, of the Smithsonian Institution, kindly provided an improvised hand-driven, double-lens camera of about 11½-foot focus; everything was packed in water-tight cases so as to be prepared for difficult landings. I decided, namely, to get, if possible, on one of the islands not occupied by any eclipse party which, while equally desirable, were not as accessible as the Tongas where all the parties congregated.

The *Annapolis* left Pago Pago, Tuesday night, April 25, and arrived at Tau Island—the nearest accessible island in the belt—the following afternoon. The entire outfit was landed without mishap through the breakers on the northwest side of the island, near the village of Tau; this part of the work was entrusted to Capt. Steffany, a well-known pilot in these waters. By the time the instruments were unpacked and assembled and suitable sites chosen, night came on. We were comfortably quartered in Vaitupu's house, the widow of Tuimanua, who died a couple of years ago and who, during his time, was the most powerful king of the Apanua group.

She, as well as the Samoan chiefs and the natives, showed us every possible courtesy and hospitality and evinced great interest in the success of our work. The party was entertained by the "village fathers" at a native feast and Vaitupu gave a siwa in our honor.

Thursday, the day before the eclipse, unfortunately, was cloudy and showery and our preparations were greatly retarded in consequence. As my chief object was to ascertain whether there might be any possible magnetic effect during the eclipse, I had to pay prime attention to the magnetic observations and to the training of an assistant, Quartermaster Urle, of the *Annapolis*, for taking readings of the magnetic declination every minute for about five hours on the day of the eclipse.

I was fortunate in being able to turn the charge of the photographic work over to Lieut. McDowell, U. S. N., in command of the *Annapolis*; he was assisted by Messrs. Reed and Steffany, also by Dr. Connor and Chaplain Pierce—all of the *Annapolis*. I made the necessary calculations for the orientation of the camera and laid out the necessary lines for guidance in the placing of the camera. Owing to the inclement weather the day before, it was not possible to get the camera finally mounted and in proper position until shortly before totality. The day of eclipse was fortunately clear throughout. There was no opportunity for trying out the finding telescope and slow motion screws in declination and right ascension.

Just before totality, Lieut. McDowell found that he could not use the finder and so rigged up a hastily constructed sighting device for keeping the sun's image centered on the plates and eliminating the diurnal motion. Two exposures of 15 seconds and two of 1 m. 10 s. were obtained. When the plates were developed, it was found that the improvised sighting device had not been wholly successful and so the photographs exhibit effects due to diurnal motion. Apart, however, from these defects, the photographs show clearly not only the inner corona but also most interesting details and coronal extensions reach-

ing out over one half of the sun's diameter. The present corona thus fulfilled the expectations of great development during a sun-spot minimum.

The mean duration of totality, as observed at shore by Lieut. McDowell and Dr. Connor, and aboard the *Annapolis* by Lieut. Baker, U. S. N., was 2 m. 1 s. The great coronal extensions which were chiefly in the sun's southwestern and northeastern edges were not seen visually, for some reason, by any member of the shore party nor by the party aboard the *Annapolis* anchored a few miles distant, in Faleasau Bay. They were, however, seen by two observers to my knowledge, in the Tongas, viz., Capt. Holford on board the *Tofua* and by Mrs. Clement Wragge, who with her husband, the well-known meteorologist, was located near Hapaii, Id.

It is greatly to be regretted that the better equipped and specially trained astronomical parties at Vavau, Tonga, were not blessed with the singular good fortune which befell us at Tau Island. For our prime work—magnetic—it would not have mattered had the weather been bad.

According to special arrangement magnetic observations simultaneous with ours at Tau were made at the five coast and geodetic survey magnetic observatories, also at Melbourne, Christchurch and Apia, where quick-run magnetograms were obtained for five hours. Until the records have been received from stations over the entire globe, it will not be possible to determine definitely whether or not the present eclipse was accompanied by any minute and temporary change in the earth's magnetism.

L. A. BAUER

THE CARNEGIE,
COLOMBO, CEYLON,
June 21, 1911

SAMUEL CALVIN

SOME weeks ago there appeared in SCIENCE a brief notice of the death, on April 17, of Professor Samuel Calvin, head of the department of geology in the University of Iowa, and state geologist of Iowa.

Samuel Calvin was born in Wigtonshire, Scotland, February 2, 1840. He came with his parents to America when he was eleven years of age. For three years the family lived on a farm in the state of New York, then they came to Iowa, where from that time until his death Samuel Calvin made his home.

He received his college education at Lenox College, Ia. When he was twenty-four years old he enlisted in the army and served for a few months in the civil war. After returning from the war, he was for four years a teacher of science in Lenox College. He resigned this position to go to Dubuque, where, for seven years, he was principal of a ward school. In 1874 he was elected to a professorship of natural science in the University of Iowa. Here, at first, he had charge of botany, zoology, geology and physiology. Later, he was made professor of geology, a position which he filled with distinction until his death.

He received from Cornell College the degrees of M.A. and LL.D., and from Lenox College the degree of Ph.D.

In the year 1865 he married Louise Jackson, of Hopkinton, Ia. She, a son and a daughter survive him.

In the year 1892 Dr. Calvin was elected state geologist of Iowa. This position he resigned in 1904 owing to the stress of other duties. However, in 1906, upon the resignation of Professor Wilder, he was again elected state geologist and continued to serve until his death. The Iowa Geological Survey under his directorship published about twenty volumes of reports dealing with the geology and mineral resources of the state. Of great scientific value have been his own contributions to the geology of Iowa, especially those papers which have added to our knowledge of the Pleistocene. His most recent scientific publications, which deal with the Aftonian mammalian fauna have done much to unravel some of the difficult problems of Pleistocene paleontology. In all his scientific work he was thorough, no details were considered trivial—his one desire was to discover truth—to find any facts which could

make knowledge clearer, broader, more definite. That he had the power to clothe his thoughts in beautiful language is clearly shown in all his writings.

Professor Calvin was a great teacher and his students loved him. His simplicity, his gentleness, his love of justice and truth, his intolerance of deceit and sham, his deep sympathy, his high regard for religion, his lofty ideals of life—these were the characteristics by which he influenced the lives of those who had the privilege of knowing him. Only such a man as he could have given expression to the following tribute to noble manhood:

Wherever noble deeds are done for truth and right; wherever weak, despairing, fainting, faltering men and women need encouragement to take up heroically the burdens and duties of life; wherever sorrow yearns for sympathy and consolation, or sickness creates necessity for tender ministrations, where the pestilence walketh in darkness; where sin, foul and loathsome, waits for victims; where overpowering temptation saps the foundations of the better will and weaves inextricable toils; wherever, indeed, many-sided humanity calls for help, there will you find some messenger of truth, forgetting self, filled with zeal for God and fellowmen, lifting, helping, encouraging, consoling; pointing out the path of wisdom and the path of peace; illustrating the importance of right living, and leading all to the true appreciation of the beauty of holiness. Such is the noble side of human nature, such is the grand side.

In the death of Samuel Calvin the nation has lost a distinguished scholar, an inspiring teacher and a true and noble man.

GEORGE F. KAY

STATE UNIVERSITY OF IOWA

SCIENTIFIC NOTES AND NEWS

PROFESSOR W. JOHANSEN, of the University of Copenhagen, whose recent work on heredity and pure lines has attracted much attention, is to give in October and November a course of lectures and seminar conferences on "Modern Conceptions of Heredity" at Columbia University, under the joint auspices of the departments of botany and zoology. Four public lectures will be given on the afternoons of October 13, 20, 27 and November 3, and

these will be supplemented by a series of about eight more technical seminars. The lectures, open to the general public, will give an outline of modern inquiries into the problems of genetics. The seminar meetings are intended for a limited group of investigators and advanced students, and will give opportunity for more critical and informal discussions of special researches in this field. A more detailed announcement will be made by the secretary of Columbia University toward the opening of the academic year.

AMONG those on whom the University of Birmingham conferred the honorary degree of LL.D. on the occasion of the annual meeting of the British Medical Association, which opened in Birmingham on July 21, are the following: Sir Francis Lovell, president of the Tropical Medicine Section; Dr. R. H. Chittenden, professor of physiology at Yale University; Professor H. Oppenheim, neurologist of Berlin; Professor Paul Strassman, assistant professor of obstetrics, Berlin; Dr. Byron Bramwell, president Royal College of Physicians, Edinburgh; Dr. J. A. Macdonald, chairman of council, British Medical Association; Dr. R. A. Reeve, ex-president British Medical Association and professor of ophthalmology at Toronto; Professor Sims Woodhead, professor of pathology at Cambridge.

THE Leipzig Seismological Society has awarded its gold Eduard Vogel medal to Dr. L. Schultze, of Jena.

DR. T. C. MENDENHALL, emeritus professor of physics in Ohio State University and formerly president of the Worcester Polytechnic Institute, has returned to the United States after a long trip abroad and a trip around the world.

DR. LESTER F. WARD, who is giving a course of lectures in the summer session at Columbia University, will sail for Norway on August 17 as a delegate of Brown University to the centennial celebration of the University of Christiania. From there he will go to Hamburg to attend the Congress of Monists which meets there September 8. He will remain abroad until October in order to attend the

Congress of the International Institute of Sociology at Rome, before which he is to read two papers on "Social Progress."

MR. DONALD F. MACDONALD, geologist to the Isthmian Canal Commission, formerly with the U. S. Geological Survey, has just returned to his headquarters at Culebra, Canal Zone, from a month's professional visit to Costa Rica. While there he made some collections of Tertiary fossils, which will be sent to the National Museum, and visited the Abangarez and Boston groups of mines on the Pacific slope of the Costa Rican Cordillera.

MR. FLOYD W. ROBINSON, formerly state analyst of the Michigan Dairy Food Department, who testified in the benzoate of soda case in the Federal Court at Indianapolis that benzoate of soda is a harmful preservative and that its use should be prohibited by law, has been dismissed as an employee of the United States Bureau of Chemistry "for the good of the service." Mr. Robinson protests against being dismissed without having an opportunity to know what charges are brought against him.

PROFESSOR JAMES FRANKLIN COLLINS has resigned as assistant professor of botany and curator of the herbarium at Brown University to accept a position as forest pathologist in the Bureau of Plant Industry.

PROFESSOR T. D. BECKWITH, bacteriologist and plant pathologist at North Dakota Agricultural College and Experiment Station, has been elected head of the department of bacteriology at Oregon Agricultural College and state bacteriologist for the Experiment Station. He will take up his duties at Corvallis, Ore., on September first.

THE Royal Society has awarded the Mac-kennon studentships for the ensuing year to Mr. T. F. Winmill, of Magdalen College, Oxford, for research in structural chemistry, and to Mr. T. Goodey, of Rothamsted Experimental Station, for research on protozoa in relation to the fertility of soil. The Joule studentship for the ensuing period of two years has been awarded to Mr. Albert Eagle, Imperial College of Science, for research on

the thermal relations of spectra of gases and on cognate subjects.

WE learn from *The Observatory* that Mr. T. F. Claxton, late director of the Royal Alfred Observatory, Mauritius, has been appointed director of the British Colonial Observatory at Hongkong. Dr. Doberck retired from the directorship of the Hongkong Observatory in 1907, and was succeeded by Mr. F. G. Figg. Mr. Claxton was appointed first assistant at Mauritius in December, 1895, and succeeded to the directorship on the retirement of Dr. Meldrum at the end of 1896.

THE International Commission on the Teaching of Mathematics will hold its meeting this year at Milan, September 18-20, under the presidency of Professor F. Klein.

DR. WILLIAM R. BROOKS, director of the Smith Observatory and professor of astronomy at Hobart College, Geneva, N. Y., discovered a comet on the night of July 20. Its position at 15 hours G. M. T. being R. A. 22 hours, 13 minutes and 40 seconds; declination north 20 degrees 57 minutes. Motion slow northwest. The comet is a fairly bright telescopic object in a 10 $\frac{1}{2}$ refractor, and is visible in the 3-inch finder.

COL. M. F. WARD, F.R.S., of Slough, writes to *The Observatory* that the church of that parish has lately been enlarged, but that funds are needed to complete the building by the addition of a tower and spire. He thinks that as Sir William Herschel's large telescope stood within 100 yards of the existing church astronomers might like to erect this spire to the memory of the celebrated observer.

AMONG eighteen civil list pensions granted by the British government during the past year are the following: Lady Huggins, in consideration of the services to science rendered by her, in collaboration with her husband, the late Sir William Huggins, O.M., £100. Mrs. Sharpe and her daughters, in consideration of the valuable contributions to ornithology made by Dr. Richard Bowdler Sharpe, and of their straitened circumstances, £90. Mrs. Conder, in consideration of

ical knowledge by her husband, the late Colonel Claude Reignier Conder, and of her inadequate means of support, £75. Mrs. Fysh, in consideration of the services to chemical and physical science of her father, the late Dr. George Gore, F.R.S., and of the circumstances in which she has been placed by his disposal of his fortune for the furtherance of science, £50. Miss Fanny Hind, Miss Flora Hind and Miss Emma Hind, in consideration of the services of their father, the late Dr. John Russell Hind, F.R.S., superintendent of the Nautical Almanac Office, to the science of astronomy, and of their straitened circumstances, £60. Dr. Charles Creighton, M.D., in consideration of his medical and biological researches, and of his inadequate means of support, in addition to his existing pension, £45. Mr. Thomas Whittaker, in consideration of his philosophical writings, in addition to his existing pension, £30.

MRS. HELENA B. WALCOTT, wife of Dr. Charles D. Walcott, formerly director of the United States Geological Survey, now secretary of the Smithsonian Institution, was instantly killed in the railway wreck at Bridgeport, Conn., on July 11. A correspondent writes: "Mrs. Walcott had been ardently and actively interested in the scientific work of her husband. In 1888 she accompanied him to Newfoundland where they worked out together the key to the succession of the Cambrian formations of the North American continent. They then crossed to Wales and studied the classical Cambrian sections. For eighteen seasons she accompanied Mr. Walcott on his expeditions in connection with geological researches in various regions of eastern and western United States and Canada. She was a most energetic collector, and was at all times an enthusiastic assistant in the scientific activities in which Mr. Walcott was engaged. Since Mr. Walcott's appointment as secretary of the Smithsonian Institution, she had been greatly interested in the development of the United States National Museum and in the general study of museum systems. She was planning to take a still more active part during the coming winter in

the social side of the scientific life of the capital. Possessed of unusual charm of person and manner, Mrs. Walcott's death is a heavy blow to a large circle of admiring friends and acquaintances."

THE death is announced of Edward P. North, a civil engineer of New York City, known for his work in municipal engineering.

MR. RALPH L. BROADBENT, assistant curator in the geological museum of Canada, died at Ottawa, on July 15, aged fifty-two years.

DR. FRANZ KRAHL, professor of bacteriology in the Technical School at Prague, has died at the age of sixty-five years.

THE formal opening of the Panama National Institute, established by the Republic of Panama, took place on June 18 amidst great pomp. The group of buildings forming the institute has been arranged and constructed after the plans of the University of Paris at a total cost exceeding one million dollars. The statues in bronze and white marble of Carrara and the luxurious display of historical oil paintings and medallions on the ceilings and walls of the main building cost over \$150,000. The four scattered buildings previously occupied by the colleges of the institute will be converted into Trade and High Schools.

THE *Carnegie* left Cape Town on April 26 and arrived at Colombo on June 9. Important errors in the magnetic charts of the Indian Ocean were found.

WE learn from the *Bulletin* of the American Mathematical Society that a Spanish mathematical society has been organized at Madrid, where its first meeting was held on April 5. J. Echegaray was elected president. The society will publish a *Bulletin* which will be in charge of C. J. Rudea, L. O. deToledo, A. Krahe and J. R. Pastor.

THE junior class in mining engineering at Case School of Applied Science, Cleveland, O., spent the month of June on an inspection trip through the west. Two weeks each were spent in Colorado and Utah. The instructors in charge of the party were Dr. A. W. Smith, professor of metallurgy; Dr. Frank R. Van

Horn, professor of geology and mineralogy, and Mr. L. O. Howard, instructor in mining and milling.

ONE of the most lofty mountain regions of the Appalachian system, recently surveyed by the United States Geological Survey, is depicted in detail in a topographic map which the Survey has just published—the map of the "Abingdon quadrangle." This map is on the scale of approximately two miles to the inch and shows an area of a little over 1,000 square miles, embracing portions of southwestern Virginia, northeastern Tennessee and northwestern North Carolina, the three states cornering in the southern part of the quadrangle. The topographic maps of the Geological Survey portray all the works of man as well as the physical characteristics of the country, and the Abingdon map indicates a region of great diversity. Part of the area is seen to be somewhat thickly dotted with villages, settlements and individual farm houses; other portions are shown as vast stretches of high mountain ranges with many lofty ridges, peaks and knobs, devoid of habitations. The larger portion of the quadrangle was surveyed by Topographer Duncan Hannegan, but other topographers who worked on the map are J. D. Forster, R. W. Berry, C. C. Gardner, R. A. Kiger and H. W. Peabody. Hundreds of miles of area were tramped over by these surveyors and scores of camps were established, thousands of sights made and hundreds of miles of level lines run. Thirty-nine indestructible iron bench marks were established, showing the elevations above sea level to the nearest foot. The line between Virginia and Tennessee, as shown on the map, was the subject of much controversy for many years. Recently, however, it was resurveyed, and it can now be easily followed by the monuments which have been placed at prominent places and by the cutting of the timber along the line. The line between Virginia and North Carolina, according to Mr. Hannegan, is of ancient date and is very difficult to follow; many of the inhabitants living close to the boundary are in doubt whether they should pay their taxes in one state or the other, as

there are no monuments, and marked trees are very scarce.

IN Bulletin 401 of the United States Geological Survey, entitled "Relations between Local Magnetic Disturbances and the Genesis of Petroleum," by George F. Becker, the condition of knowledge with reference to the origin of petroleum and other bituminous substances is reviewed. Some oils, says Mr. Becker, are undoubtedly organic and some are beyond question inorganic. They may have been derived from carbonaceous matter of vegetable or animal origin, and they may have been derived from carbides of iron or other metals. It is also barely possible that the hydrocarbons exist as such in the mass of the earth. While studying the subject, Mr. Becker was led to inquire whether any relation could be detected between the behavior of the compass needle and the distribution of hydrocarbons. Not much could be expected from a comparison of these phenomena, for magnetite exerts an attraction on the needle whether this ore occurs in solid masses or is disseminated in massive rocks; moreover, many volcanic rocks possess polarity. In glancing over a map of the magnetic declination in the United States Mr. Becker found that the irregularities of the curves of equal declination of the compass were strongly marked in the principal oil regions. The most marked agreement is found through the great Appalachian oil field, which is the area of greatest variation in declination. In California, also, strong deflections accompany the chain of hydrocarbon deposits. These observations are to some extent also supported by conditions in the Caucasus, where great magnetic disturbances exist. While the theory of the inorganic origin of the hydrocarbons is not proved by this study, yet the contention that great oil deposits are generated from iron carbides is strongly borne out by a study of the map of magnetic disturbances in the United States. The map shows that petroleum is intimately associated with magnetic disturbances similar to those arising from the neighborhood of substances possessing sen-

sible magnetic properties, such as iron, nickel, cobalt and magnetite.

THE Journal of the American Medical Association states that under the supervision of the health department of the Canadian conservation commission, Canada is to have established in the immediate future, a national laboratory for the manufacture of sera, vaccines, toxins and antitoxins. A subcommittee of the federal cabinet has approved of the proposal and has recommended speedy provision for the construction and equipment of the laboratory. This proposition has been endorsed by the Canadian Medical Association at several of its recent meetings.

We learn from *Nature* that a conference was held recently in the zoological laboratory of the University of Utrecht for the purpose of founding an International Embryological Institute. Austria, Belgium, England, France, Germany and Holland were represented at the meeting by workers in the domain of vertebrate embryology; and letters were received from Switzerland and the United States in support of the scheme adumbrated by the conveners of the meeting. Professor R. Bonnet, of Bonn, was elected first president of the institute, and it was decided that the first aims of the new institution should be (1) the collection of complete series of well-preserved embryos of every mammalian order, and (2) a more intimate cooperation between embryologists, for the purpose of attaining a uniformity in nomenclature and the solution of the special difficulties in this field of investigation.

"THE Production of Fuller's Earth," by Jefferson Middleton, of the U. S. Geological Survey, has been published as an advance chapter from "Mineral Resources of the United States, 1910." The fuller's earth resources of the United States, says Mr. Middleton, have attracted considerable attention for several years because of the increasing demand for this material for use as a clarifying agent for mineral and vegetable oils. The original use from which it derives its name, the fulling of cloth, is now of minor impor-

tance. For a great many years fuller's earth was imported from England, the only known source of supply, but in 1893 it was by accident discovered in this country. At Quincy, Fla., an effort was made, without success, to burn brick on the property of the Owl Cigar Company. An Alsatian cigar maker employed by the company called attention to the close resemblance of this clay to the German fuller's earth. As a result of this suggestion, the clay was tested and found to be fuller's earth, and the industry was developed. The principal use of fuller's earth in this country is in bleaching, clarifying, or filtering of fats, greases and oils. The common practise with mineral oils is to dry the earth carefully after it has been finely ground, and run it into long cylinders, through which the crude black mineral oils are allowed to percolate very slowly. As a result, the oil that first comes out is perfectly water white and much thinner than that which follows. The oil is allowed to continue percolating through the earth until the color reaches a certain maximum shade. Then the fuller's earth itself is clarified by a steaming process and used over again. With vegetable oils, however, the process is radically different. The oil is heated beyond the boiling point of water in large tanks, from 5 to 10 per cent. of its weight of fuller's earth is added, and the mixture is vigorously stirred and then filtered off through bag filters. The coloring matter remains with the earth, the filtered oil being of a very pale straw color. American fuller's earths are better adapted than the English earths for use on mineral oils, but the English earths are superior for the treatment of fats and vegetable oils. In clarifying vegetable and animal fats with American earths a more or less disagreeable taste is left—just why has never been determined. To show the growth of the American industry it is only necessary to state that from 6,900 tons in 1895 the production increased to 33,486 tons in 1909. This was the maximum, the output for 1910 being 664 tons less. Florida was the leading producing state in 1910, furnishing 57.38 per cent. of the total output. The other producing states, named

in the order of their rank in output and value in 1910, were Georgia, Arkansas, Texas, California, Massachusetts, South Carolina and Colorado.

UNIVERSITY AND EDUCATIONAL NEWS

SIR WILLIAM MACDONALD has completed a large purchase of land on the slope of the mountain adjoining Mountroyal Park and will give the property to McGill University. A new campus and residential buildings will be established upon it. The purchase price was over \$1,000,000. Including the cost of Macdonald College and its endowment, this brings Sir William Macdonald's total gifts to McGill University to \$10,000,000.

THE New York legislature has passed a bill to appropriate \$10,000 for the establishment of a school of sanitary science and public health at Cornell University.

MR. R. C. FORSTER has made a further gift of £30,000 to the fund for providing new chemical laboratories at University College, London.

AT Cornell University Dr. D. C. Gillespie has been appointed assistant professor of mathematics. Mr. G. W. Nasmyth has been appointed instructor in physics and Mr. J. Mackenzie instructor in economic geology.

DR. ELLIOT R. DOWNING, in charge of the department of biology of the Northern State Normal School, Marquette, Mich., has been appointed assistant professor of natural history in the school of education of the University of Chicago.

DR. THOMAS L. PORTER, who has been assistant in physics in Northwestern University and Clark University, has been appointed professor of physics in Colorado College.

DR. BENJAMIN F. LOVELACE, professor of chemistry in the University of Alabama, has been elected associate professor of chemistry in the Johns Hopkins University. Dr. Stewart J. Lloyd, adjunct professor of chemistry and metallurgy, has been promoted to the professorship of chemistry in the University of Alabama.

DISCUSSION AND CORRESPONDENCE

ON "SOMA INFLUENCE" IN OVARIAN
TRANSPLANTATION

TO THE EDITOR OF SCIENCE: May I take space in your columns for a brief discussion of the matter which Professor Guthrie presents in your issue of May 26, the diametrically opposite conclusions as regards the effects of ovarian transplantation reached, on the one hand by himself and, on the other, by Dr. Phillips and myself?

Beyond the point of clearly stating the essential difference in our conclusions and the ground on which this difference rests, I take it, neither Professor Guthrie nor I would care to go in the way of discussion.

The question at issue is first of all one of the sufficiency or insufficiency of evidence. Guthrie says regarding his own work (p. 816): "The primary object of the experiments was to determine if an engrafted ovary might retain its reproductive function. . . . And incidentally information on soma influence was secured." The incidental result happens to be the one of more general interest, but is impossible without the survival and functioning of "an engrafted ovary." So that the whole discussion narrows itself down to this: Has Guthrie presented adequate evidence that in his experiments an engrafted ovary *did* survive and function?

The facts are these. He transplanted the ovary of one hen into another hen. The second hen afterward laid eggs. Does it follow that the eggs came from that transplanted ovary? Not unless it can be shown that there was no other possible source from which they could have come.

What should we say to this sort of evidence? A boy rushes into the house. "Father," he says, "I have killed a hen." "How do you know, my son?" "Why, I threw a stone over the fence into the henyard, and when I opened the gate and went in, there lay a dead hen." Is that proof that the hen was killed by the stone which the boy threw over the fence?

To prove Guthrie's conclusion two facts

must be established neither of which has he made any attempt to establish. These are, first, that the introduced tissue survived; and second, that no other ovarian tissue was present in the engrafted animal. Our own experiments show that in guinea-pigs engrafted ovarian tissue *taken from another animal* survives in only a small percentage of cases, and further that complete castration of the female is difficult. Even though every apparent vestige of the ovary is removed, nevertheless a functional ovary may later be developed at the original ovarian site. This possibility for fowls Guthrie ignores, yet in fowls complete castration would seem to be a much more difficult matter than in guinea-pigs, because of the diffuse nature of the ovary and its close adherence to large blood vessels. It seems to me essential to Guthrie's contention that he should establish his ability completely to castrate a hen. This he has not done. For if the hen can not be castrated, what warrant have we to speak of somatic influence in the offspring of grafted hens, these offspring being of doubtful origin? I can think of only two ways in which the survival of engrafted ovarian tissue could be established, viz., (1) by transplanting the ovary into some situation other than the normal one and subsequently demonstrating its existence there by autopsy and histological examination. This, the most direct and certain method, we have used with success in a number of cases, as have also several earlier investigators, whose work has been reviewed by Dr. Phillips and myself. Guthrie is prevented from employing this criterion by his uniform practise of transplanting the ovary to the original ovarian site. There is left to him only the criterion next to be mentioned, viz., to judge by the character of the young produced by the grafted animal. He finds in general that the young strongly resemble the grafted mother. Now, this fact admits of two interpretations, one of which Guthrie offers; the other has been offered by Phillips and myself. Guthrie holds that the introduced ovarian cells changed their character to conform with that

of the animal in which those cells were contained; we hold that it is unnecessary to assume such a change, but that the young were like the mother because she *was* their mother, and that they developed from *her own ova*, not from those introduced. We have shown elsewhere by a detailed examination of the facts reported by Guthrie that there is nothing in them at variance with the known facts of color inheritance in fowls, if it be supposed that in these experiments the mother furnished her *own ova* to produce offspring. But if it be supposed, as Guthrie does, that the ova came from an engrafted ovary, then serious contradictions are encountered as regards the color inheritance. Such contradictions Guthrie may not lightly push aside by disclaiming any interest in laws of inheritance on the ground that they are of "no concern" to him. He who claims to have modified inheritance should know what *normal* inheritance is, and he can not divert attention from chickens by scornful references to "peas," nor from stubborn facts by thrusts at "theories built largely upon speculation." No theories are involved in this discussion except the one which Guthrie has propounded, that inheritance is affected by foster-mother influence. We are concerned merely with facts which may either substantiate or disprove this hypothesis. It happens that the subject of color inheritance in fowls has been an object of careful study by several competent observers for a number of years, and we have a large body of data on the normal inheritance of black and white in crosses of fowls. Is it wise in discussing a supposed case of modified color inheritance in fowls to disregard this data as of "no concern"? Is breeder's evidence of "no concern" in a question of modified breeding?

To sum up in a few words our criticism of Guthrie's "evidence of soma influence," we hold that no satisfactory evidence of such influence has been produced because first, it has not been shown that a hen can be completely castrated, but if this can not be done, we can not be certain that eggs discharged from the ovary were really derived from intro-

duced tissue and not from a regenerated ovary. Secondly, it has not been shown that in Guthrie's experiments the transplanted tissue actually persisted. Without the fulfillment of *both* these conditions no transplantation experiment can be considered critical.

Guthrie calls attention to the fact that in an early announcement of his results he drew only provisional conclusions. This is quite true; they were in their entirety as follows:¹

1. "The ovaries transplanted in these chickens seemed to function in a normal manner."

2. "The color characters of the resulting offspring appeared to be influenced by the foster-mother."

No exception can be taken to these modest conclusions. No claim is made in them of more than a *seeming* persistence of engrafted tissue and an *apparent* modification of the color characters of the offspring, which however at the present time we are in a better position to explain.

If we are to understand that in the present paper Guthrie means merely to reassert these original conclusions, I make no objection to them.

It did *seem*, as Guthrie stated, that in his experiments the transplanted ovaries functioned, but that is no proof that they *did*. Our criticism of Guthrie's results is directed merely toward establishing this point. Doubtless it *seemed* to the boy who threw the stone into the poultry yard that he had killed the hen, but I doubt whether his father would have accepted that conclusion without some independent investigation. Such an investigation of Guthrie's results, Phillips and I have made.

In a case which we have fully described elsewhere the two criteria of the persistence and functioning of transplanted ovarian tissue which have been enumerated are, I believe, adequately met. That Guthrie does not share this view is of little consequence in this connection, but in stating his reasons for dissent Guthrie, doubtless inadvertently,

¹ *Journ. Exp. Zool.*, 5, p. 571.

misstates certain facts which I can not pass over in silence, lest this be interpreted as assent. He states, first, that we used "mongrel stock." "Therefore, any evidence furnished by the character of the offspring would be of doubtful value." On what Guthrie bases this statement I am unable to discover. It is wholly contrary to fact. We described in the body of our paper "one successful case" and in a postscript a second case complete except as regards the autopsy. In describing the successful case, p. 8, the statement is expressly made that the ovary was taken from "a pure black guinea-pig." This guinea-pig belonged to a family of coal-black animals which I have had for about seven years. This family is descended without admixture of other blood from three original individuals, a male and two females, all intensely black, the progeny of which have been closely inbred now for several generations without ever producing any observable deviation from the solid black type of the progenitors. The albino grafted was also of pure race, one which I have bred for about ten years. The albino male with which the grafted animal was mated was of a different strain, but of known and tested gametic composition, so that I can state with much positiveness the kind of young which he produced (and would regularly produce) in matings with guinea-pigs of different color varieties.

The second successful case, described in a postscript, concerned a color variety which I originated, the brown-eyed cream, and which breeds very true, since all the color factors which it contains save one are recessive in nature. This variety *can* produce only one variety of colored young. It is the *ultimate recessive* colored variety of guinea-pig. Having originated this variety some years ago and bred it pure and in crosses ever since, I think I may justly claim to know something about its behavior in inheritance. In neither of the cases which we have described as "successful" was an animal used whose breeding capacity was not definitely and fully known, as definitely as we know what will happen when oxygen and hydrogen are combined.

The charge of "mongrel stock" is therefore groundless.

Guthrie's second criticism of our evidence is this, "It is not proved that the offspring may not have come from ovarian tissue of the host left in site after operation." But both the grafted animals were albinos and they were mated only with albino males. In all recorded cases, of which I have myself observed many hundred, albinos so mated produce only albino young. Had ovarian tissue been left in site after operation and liberated ova which developed, these should have produced *albino* young. But these grafted albinos, which had received an ovary from a colored animal, produced *colored* young, in each case of the particular color type that characterized the animal furnishing the graft. Is there really then any uncertainty about the source from which the functioning ova came?

W. E. CASTLE

LABORATORY OF GENETICS,
BUSSEY INSTITUTION,
HARVARD UNIVERSITY,
June 21, 1911

MEASURING THE MERIT OF ENGLISH WRITING

TO THE EDITOR OF SCIENCE: Professor Thorndike's article in SCIENCE of June 18 on a scale for measuring the merit of English writing, seems to parallel the old question: "Which is best, a pair of scissors or a pair of tongs?"

To have any value as a test of merit the writing of "pupils in their teens" should be comparative, and you can not properly compare paragraphs based on different topics, recollections or quotations from school readers, and attempts at expression of totally distinct emotions.

One method which might approximate to a basis of comparison would be to require from all the pupils a paraphrase of one single paragraph, as far as possible to be expressed in entirely different words from the original. Even this would be subject to the objection that a child writes best when it writes of something it naturally appreciates, and in which its interest is not forced; and the same

bit of writing would rarely appeal to any large number of children in an equal degree or in the same way; consequently their relation to it would not be of a strictly comparative kind in a literary sense.

The examples given seem to me absolutely valueless for comparison. Number 607 is the production of an idiot. Number 520 is a quotation; no child in its teens could have conceived it. Number 434, if a genuine original, is the only one showing anything but a lesson poorly remembered, it is the only one not quoted or paraphrased from an adult production which has any literary merit at all.

WM. H. DALL

SMITHSONIAN INSTITUTION,
June 19, 1911

GENOTYPES ARE THE SPECIES UPON WHICH GENERA ARE BASED

THE case presented by Dr. Stiles on page 620 of *SCIENCE* for April 21 last, possesses exceptional importance for the student of muscoid flies. Probably in no other superfamily of animals have as many misidentifications been made as in the Muscoidea. Species have been repeatedly confused, combined, jumbled and wrongly determined ever since the time of Meigen, if not before, until the tangle has now become frightfully intricate in character. Especially within the past decade or two have misidentifications of North American forms enormously increased, so that the literature is now overburdened with the resulting error, from which it will be a labor of great magnitude to free it.

The principle involved in misidentifications or cases of mistaken identity is always the same for all cases, and the problem is capable of only one correct solution. Of two diametrically opposed propositions, one must necessarily be right and the other wrong. While I can see the case clearly from both points of view, the wrong premises of the one view stand forth distinctly in my mind, and I can not grant that there exists here any necessity for arbitrary decision. The whole matter rests, of course, upon the adoption of rational and correct premises.

Properly approaching the question, its solution is simple, and I need only repeat here the axiomatic title at the head of these remarks.

The correct and only logical premises are represented in the axiom that EVERY RECORD OF A SPECIES OR OTHER TAXONOMIC UNIT IN THE LITERATURE BECOMES AT ONCE A PART OF THE SYNONYMY OF THE SPECIES OR UNIT INTENDED FOR RECORD BY THE RECORDER. It makes no difference under what name the record be made, the entity referred to remains the same, and the synonymy of that entity is thereby enriched by the name used followed by the name of the author making the record together with the date of same. This precludes confusion whether or not misidentification exists. The genus *X-us Jones, 1900*, unmistakably has for its type, under the conditions of the problem as stated, the species *albus Jones, 1900*. The genotype can be no other than this, which is the particular form so identified by Jones at the time and by him intended as the type of his genus. Jones has misidentified his genotype with Smith's species, hence the name *albus Jones, 1900* (*non Smith, 1890*), becomes a synonym of the name that shall finally hold for the genotype, that is to say, the particular form indicated by Jones. It is conceivable that Jones might differently identify the same form at different times, hence the necessity for a synonym to take the date of publication, which should include the month and day if Jones is a voluminous and frequent publisher.

The fallacy of the opposite premises is very evident. Were we to admit the latter it would be impossible to present a rational synonymy of forms. In the above case, *albus Smith, 1890*, has no further connection with the matter in hand after it has been proved that *albus Jones, 1900*, is a different form. It should be evident that an author's record of a form must remain always a record of that form in his sense at the time of record. The name he uses is merely a handle by which we can ourselves find and locate that form. If we ever decide that a record of a form is *not* a record of the form in the sense

intended to be recorded, we are clearly on the wrong road. And this is exactly where we should be were we to decide in the above stated question that the record of albus Jones, 1900, is not a record of albus Jones, 1900, but a record of albus Smith, 1890, knowing the contrary to be the case. The wording of the question itself in Dr. Stiles's title carries the correct solution. The species upon which a genus is based is necessarily the type of that genus. If it be found that the species has been erroneously determined, the determination must be corrected, and if it is found to be undescribed it should be at once characterized by the discoverer of the erroneous determination or some one else; otherwise the genus might by some be held to fall, being left without a described type species that can be designated. I would suggest that a special provision be made for such cases, whereby the genus need not fall in event of its type species proving undescribed. It can always be referred to by the name used in the original record, as albus Jones, 1900 (non Smith, 1890), until it can be better characterized. The species, whatever it prove to be, remains the type in the end.

Suppose the case of A and B, two men who are look-alike twins. I am acquainted with A, but I am ignorant of the existence of B. I see B, whom I believe to be A, commit a crime, and I give evidence in court, in my mistaken but conscientious belief, due to a misidentification of individuals, that A committed the said crime. Does this make A the criminal in the case, or does B remain the criminal? I think no argument is needed to show clearly that the person whom I saw commit the crime is bound to remain the criminal in the case, regardless of the name by which I designate him; my A is synonymous with B. Entities must be maintained. If individuals are confounded, their individuality is lost.

Following still further the principle of mistaken identity, it is evident that an author can not correctly put a previously published record into his synonymy without correctly ascertaining the identity of the forms con-

cerned. It is equally evident that, whether he has or has not correctly ascertained the same, he personally, and no other, is responsible for the synonymy published under or over his name. Still further, it is evident that, if his synonymy be found incorrect, it does not hold, and the status of the particular forms which he has wrongly so indicated remains the same as before. No synonymy is entitled to recognition unless founded on material studied, hence the detection of error carries with it a location of the material under consideration at the time by the said author. If the points involved in the same ever become of sufficient importance to warrant, then the forms represented in the said material must properly, for synonymic purposes, take the names by which the said author recorded them plus his own name and date.

The element of protection demands consideration. It is evident that a taxonomic unit once correctly defined and named must be recognized and protected from distortion. What protection has albus Smith, 1890, if we allow it to be cited as the type of a genus that not only was manifestly not intended for it by its author but may even prove to be incompatible with it in its characters? If the characters of the genus X-us Jones, 1900, are not stated by its author, the same are to be found only in the material of albus Jones, 1900. If no such material has been studied and the new genus has been proposed on the strength of the description of the genotype cited, then no misidentification exists and the case as stated does not apply. Likewise if the type material of the genotype is cited the case does not apply. All phases which do not carry the misidentification principle may be similarly eliminated from the present consideration.

Those who would maintain, in the face of the above remarks and under the conditions of the question, as stated by Dr. Stiles, that albus Smith, 1890, is the genotype of X-us Jones, 1900, can in my opinion have no other excuse for their action than the desire to shirk taxonomic responsibilities because they involve increased labor. Clearly an author

has no right to treat a subject in the literature without complying with the responsibilities which his treatment, so far as it goes, demands. If he does so, he alone is at fault and he alone must suffer. Slipshod taxonomic methods carry their own germs of decay. If I myself have offended in this respect, I neither deserve nor desire sympathy as to the particular points of my offense. Every author's work must be verified until it becomes apparent that correctness has been attained. In this manner only can we put taxonomy on a sound basis. It is evident that the desired consummation of demonstrated taxonomic correctness for most forms is a long way off; but deplorable as this may be, and as difficult of achievement as it is deplorable, we can not in any event justly dodge the points at issue. Nomenclatorial problems must be fairly met or we shall never attain the desired end.

I have heretofore held aloof from discussions of nomenclatorial intricacies in general, knowing that the conditions of muscoid taxonomy are at present such that few cases can yet be definitely stated, although the future holds a multitude of them for ultimate solution. But I consider that the necessity for deciding the present question as above suggested is of such paramount importance to the welfare of future taxonomy that I have, at the risk of prolixity, presented the evidence both direct and indirect as fully as I am able to see it at the present time. The effect of the final decision by the international commission of questions involving the misidentification principle will have the utmost bearing on muscoid taxonomy, from which confusion will never be eliminated until we know the morphology of the reproductive system, egg and early stages thoroughly, as well as every detail of the external anatomy of the fly, and perhaps all the details of its internal anatomy. The conditions in the Muscoidea are quite unique, forms belonging to distinct genera and tribes, or even distinct subfamilies, often being closely similar in external adult structure. Many authors have in consequence sadly mixed and confused distinct forms throughout their work, and if

we ever decide against the *intent* of an author it goes without saying that we shall be irretrievably lost in muscoid synonymy. Correct interpretation of an author's meaning is as important to us as priority in nomenclature. Therefore the importance of securing a rational working decision can not be overrated.

CHARLES H. T. TOWNSEND

PIURA, PERU,
May 7, 1911

LATIN DIAGNOSIS OF FOSSIL PLANTS

AMONG the rather numerous nomenclatorial rulings of the International Botanical Congress which are considered retrogressive by a large number of systematists is that which requires the diagnoses of new species, genera, etc., to be in Latin (*sic*).

In order to test current opinion among paleobotanical workers a memorandum has been circulated by Professor Nathorst, of Stockholm, and Mr. Arber, of Cambridge, and the result, published in a recent number of *Nature*¹ will be of much interest to American systematic botanists.

The rather remarkable result of this interchange of opinion shows that every paleobotanist in Scandinavia, Great Britain and North America proposes to disregard this ruling of the congress.

The memorandum which was circulated contained the following statements of intention:

1. I do *not* propose to include a diagnosis in Latin in the description of any new species, genus or family that I may institute in the future, unless there appear to me, in particular cases, to be special reasons for so doing.

2. I will *not* refuse to accept new species, genera or families of fossil plants instituted by other workers in the future, solely on the ground that their description is not accompanied by a diagnosis in Latin.

This was signed, with some modification of wording in the case of Mr. and Mrs. Clement Reid and Professor Seward, by the following

¹ May 18, 1911, pp. 380, 381.

students: Nathorst, Bartholin and Halle, of Stockholm; Benson, Royal Holloway College; Berry, Johns Hopkins; Cockerell, University of Colorado; Gordon, Edinburgh University; Hartz, Copenhagen; Hickling, Stopes, Watson and Weiss, of Manchester; Holden, Nottingham; Hollick, New York Botanical Garden; Jeffrey, Harvard; Kidston, Stirling; Knowlton and White, of Washington; Lewis, Liverpool; Maslen, Oliver and Mr. and Mrs. Clement Reid, of London; Möller, Sweden; Mr. and Mrs. D. H. Scott, Oakley, Hants; Arber and Thomas, Cambridge; Wieland, of Yale.

Judging by the protests one hears in the United States and the accounts of the Botanical Congress, it would appear that a good many of the rulings which it adopted are very far removed from being international in character or origin. Certainly its proposals regarding fossil plants, which emanated for the most part from Berlin, did not display much insight into the subject.

EDWARD W. BERRY

EXECUTIVE RESPONSIBILITY

TO THE EDITOR OF SCIENCE: May I trespass on your space to the extent of replying briefly to the criticism by Professor A. D. Mead in your issue of June 23, of my letter on academic tenure, which was printed in SCIENCE on May 12?

Professor Mead's criticism has such a moderate tone, and there is so little in it that at all affects the tenability of my position, that it would not demand a reply were it not for the fact that it seems to imply that the "freedom of opinion and utterance" he declares to be so well guaranteed to the Brown faculty, should not extend to the columns of SCIENCE as well; and that it has other implications which suggest the workings of the theological rather than the scientific mind, by a reliance on dogmatic assertion instead of evidence.

Professor Mead admits that men have been removed from the Brown faculty, but declines to enter into any "futile controversy" over the cause. They must have been removed

justly, he argues, because the charter forbids such action for anything else than "misdemeanor, incapacity or unfaithfulness," and the present administration only enforces it for such reasons. This may be the case, but we have no evidence of its being so except Professor Mead's opinion; and that is offset, in my mind at least, by the assurance of several present and past members of the Brown faculty, that tenure is extremely uncertain there, and that arbitrary removals are frequent.

There are always two sides to any question, and it would be unjust to accept the statements of men who have been removed from the Brown faculty, as unbiased evidence. The statements of such men, however, that I have heard made with increasing frequency during the last few years, go far to call into question, if they do not disprove, the assertion of Professor Mead that men of long service in the university are not removed until they are given a "reasonably fair chance of readjustment in other positions." Of course there is room for difference of opinion as to what constitutes a reasonably fair chance; but I question very much if, even after the statements of these men had been much discounted to allow for personal interest, an ordinary jury would agree that they had had much of anything in the way of a chance to readjust themselves in new positions.

Leaving out of the question, however, the statements of men who have been removed, a case is made out against the Brown administration by the very arguments with which Professor Mead tries to justify its course. He admits that men have been appointed to various professorial grades and continued in them for years, only to be removed afterwards by the same administration that advanced them. Such a course as that can not be justified, and the attempt to do so by statements about "having reached the limit of growth in the environment of the particular institution," should be very severely reprehended by everyone who desires to save education from serious discredit. Even in our largest institutions too much is said about the necessity for rare and special talents, and in Brown,

where the problems of instruction are less varied, such remarks have even less justification. The qualities that make a successful teacher, in any environment, are high character and wide knowledge; and a very few years trial should suffice to inform the discerning and disinterested judge whether or not a man possesses these essentials. If he does not there is no justification for retaining him, no matter how much money or inconvenience is saved by doing so. If he does there is no justification for removing him, no matter how much money doing so releases for other purposes, or how much the administration believes in his incapacity, so long as it has no other evidence of it to offer to the public except vague general statements about environmental unfitness, and having reached the limit of growth. Such statements are based altogether too much on personal opinion and on intangible, esoteric considerations to justify action so serious in its consequences as removal from an academic position always is.

SIDNEY GUNN

MASSACHUSETTS INSTITUTE
OF TECHNOLOGY,
July 3, 1911

ACADEMIC AND INDUSTRIAL EFFICIENCY

TO THE EDITOR OF SCIENCE: Referring to Mr. Handschin's letter concerning academic and industrial efficiency in your issue of June 9, I feel that it should be said that it is very doubtful whether the efficiency of educational institutions can be compared in any way with the efficiency of industrial concerns.

I very much doubt the unsupported thesis: "But the institution which pays the most to 'productive' labor is the most efficient." If a railroad were to be built by hand labor the labor cost would be relatively high, but I fancy no one would say that the work was efficiently done. Indeed, it may be stated with almost no other support than our general knowledge of things that in proportion as new machinery is devised to take the place of hand labor the efficiency of production is increased.

In general, efficiency, as the word has been recently used, is the ratio of useful energy of one form recovered to total energy of another form supplied or destroyed. I should like to inquire who can measure the total energy supplied by a teacher or the useful energy recovered?

Without question there are certain economies that may be realized in the conduct of an educational institution of any kind, but, while these economies must not be overlooked, they are the least important of all of the items to which attention should be given. In most of the discussion that has appeared it has seemed to me that the duties of the college and the university have been confused. Whatever may be the dictionary definition of a university, it is accepted as a place for research, a place where enthusiastic men may find encouragement and the means to assist them in their efforts to increase the world's store of knowledge. It is not necessarily an aggregation of colleges—it is not a commercial laboratory. Its duty, therefore, is to promote research with only so much control by a group of scholars as to make it reasonably certain that any study undertaken is worthy of effort. It is the duty of a college to give young men and young women a certain small proportion of knowledge already available, to teach them where and how to get more, and to endeavor to inspire them with a high sense of duty to their country, their neighbors, themselves and their God. This is as we know the college in America.

In its mechanical or commercial sense efficiency is not a word to be used in connection with this duty of the college, or the work of a university. The cost matters little if the duty and work are well performed.

I would not have this statement considered as a reflection upon the excellent report of Mr. Cooke to the Carnegie Foundation for the Advancement of Teaching, which report seemed to me to be full of suggestions of great value.

WM. G. RAYMOND

IOWA CITY, IOWA

THE METHODS OF A VETERAN INVESTIGATOR AND
TEACHER

TO THE EDITOR OF SCIENCE: It occurs to me that some of my scientific colleagues may be interested in the following statement of what I regard as the most important educational and scientific outcome of fifty years of study and forty-two of teaching: (1) All parts of a given animal should receive one and the same serial number. (2) Slips should be used for promptly recording new observations, references, ideas, and all data (*e. g.*, localities, donors, modes of preparation) not ascertainable from the specimens themselves. (3) Beginners should be taught correct methods by explicit directions. (4) Before lecturing upon a species or a group there should be shown a specimen or a representation of one. (5) In all composition the following should be sought in the order named: clearness, consistency, correctness, conciseness, completeness. (6) Published errors should be promptly corrected. (7) All natural classification is dichotomous. (8) For the study of the structure, development, succession and relationships of vertebrates the best group to begin with is the Selachians, the sharks and rays; if several forms can be studied the first should be—and if but one, that one should be—the acanth or “horned dogfish,” *Squalus acanthias*. (9) The objective study of the brain should begin in the primary school; the pupil himself should expose, draw and dissect the brain of the acanth shark; with successive appropriate changes as to forms and methods the high school graduate should have gained as much real knowledge of the human brain as is now possessed by the average graduate in medicine.

BURT G. WILDER

CORNELL UNIVERSITY,
June 20, 1911

QUOTATIONS

THE DEPARTMENT OF AGRICULTURE AND
DR. WILEY

It begins to look pretty clear that the real problem before the President in connection

with the Wiley affair is how to let it drop with the least amount of disturbance and inconvenience. This does not imply that he will decide the matter without looking into its merits. His decision will not be made until he has personally examined the record. But it requires neither a gift of divination nor a preternatural command of legal intricacies to predict with a great degree of confidence that the recommendation made by the personnel board of the Department of Agriculture, and approved by Attorney-General Wickersham, will not be followed by Mr. Taft. Every day that has passed since it was made has strengthened not only the belief that the punishment proposed was utterly disproportionate to the alleged offence—even supposing that offence to have been of precisely the character asserted—but also the impression that the President is quite as well aware of this as anybody. The Washington news, in papers of all shades of opinion, has been steadily pointing in the direction of a smoothing over of the affair—not for Dr. Wiley, but for Mr. Wickersham.

Before the matter goes further, and the initial stages of it become hazy in the public mind, it is well to recall just what Attorney-General Wickersham did in the case. The personnel board of the Department of Agriculture had found that in the arrangement made by Dr. Wiley with Professor Rusby, an eminent pharmacological expert, the terms of a law limiting the compensation of experts employed by the Agricultural Department were violated. It was not alleged by anybody that Professor Rusby had been overpaid for his work; it was not alleged by anybody that Dr. Wiley's object in securing his services was anything but that of getting the best possible results for the government. The charge was simply that the law made \$4,000 a year the maximum pay for an expert, that it had been decided that this means that the *per diem* pay of an expert shall not exceed \$11, and that Dr. Wiley had made an arrangement for an annual compensation of \$1,600 to Pro-

fessor Rusby, in such a way as to result in his getting a *per diem* compensation greater than this obviously inadequate one, for the days that he gave up to the work. Now, nobody would have complained if Mr. Wickersham had informed the President that this is a violation of the law. Nobody would have found fault with him if he had expressed his opinion that such violation was a serious matter. But when he went outside his province as a lawyer and told the President that in his judgment this disregard of a peculiar regulation, in so small a matter, and without the slightest trace or suspicion or hint of bad motive, was sufficient reason for approving a recommendation calling for the resignation of a faithful public servant, filling with exceptional zeal and devotion an office of unusual importance, he invited just such criticism as he has been subjected to in the past few days.

In deprecation of such criticism, the curious point is now put forward that Mr. Wickersham's report was not intended to be made public, but was designed solely for the President's private information and guidance. This may be a good point for Mr. Taft himself to fall back upon, but it is difficult to see how it can do anything for Mr. Wickersham. If the report was one for the President's private ear, the President might, to be sure, throw it into the waste-paper basket; but that can not have been the purpose for which it was originally destined. So far as in him lay, Mr. Wickersham backed up the personnel board's recommendation; and it is impossible to see wherein there is any less demerit in the advice to do an act of injustice because the advice was given in secret. To most minds, we fancy, that is an aggravation of such an offence, not an extenuation. And it is impossible not to recall the fact that in the unhappy muddle over the Lawler memorandum in the Ballinger case, in which Mr. Wickersham bore a conspicuous part, a bungling policy of secrecy was responsible for the worst of the trouble.

There is another analogy between the present affair and that of the Ballinger-Cunningham-Pinchot difficulty which the President will do well to bear in mind. In this case, as

in that, there are two aspects which the subject presents; in this case, as in that, everything depends upon maintaining a sense of proportion as between these aspects. There is the narrow view of the mere lawyer and the mere disciplinarian; there is the broad view of the man responsible for large and difficult affairs. It is not necessary to ignore the requirements of law or even the exactions of red tape in order to do justice to the larger things. But it is one thing to insist that even the most zealous and well-intentioned of officers must obey the law; it is quite another thing to permit the enemies that such officers are constantly making to seize upon little errors, or technicalities, or violations even of official etiquette, as a means of getting them out of the way. Such work as that of fighting land thieves or food adulterators demands enthusiastic zeal and inexhaustible energy; if you are going to make the situation impossible for a man who has these qualities unless he combines with them an immaculate record upon every technical point, you might as well surrender at once to the land-grabbers and the adulterators. And it is because the plain people understand this that they insist upon any such affair as the Ballinger case or the Wiley case being uncovered from top to bottom. Any attempt to confine it within narrow or technical bounds is sure to fail.—*New York Evening Post*.

SCIENTIFIC BOOKS

Contributions to Medical Science. By HOWARD TAYLOR RICKETTS. Chicago, University of Chicago Press. 1911. Pp. ix + 497. \$5.33.

The committee of the Chicago Pathological Society which was intrusted with the office of preparing a suitable memorial of Howard Taylor Ricketts have issued a memorial volume containing many of the chief original studies of this remarkable investigator.

The volume opens with a brief and dignified statement by Hektoen of the main events of Dr. Ricketts's career, ending in his untimely death in Mexico City from the deadly Mexican typhus, the disease whose secrets he

was pursuing. Then follows the well-known and now classical study by Ricketts on "Oidiomycosis of the Skin," and an important contribution by Benjamin F. Davis on "The Immunological Reactions of Oidiomycosis in the Guinea Pig," a work which grew out of and is partly based upon the observations of Ricketts. One is then reminded, by several articles, that Ricketts made important contributions in the field of immunity, in studies on lymphotoxic and neurotoxic sera, and on tetanus.

The main portion of the volume consists of the remarkable series of papers on Rocky Mountain fever, in which is found the history of the various steps which led to the unravelling of the mysteries of this disease. Some unfinished studies relating to the mode of transmission of the disease were taken up by Davis, Petersen, Moore and Maver, and their reports follow. LeCount contributes, with many illustrations, a report on the pathological anatomy of the disease based on the material collected in six autopsies performed by Ricketts. Finally come the preliminary reports of Ricketts and his colleague Wilder of their studies on Mexican typhus, in which they were able to show that the disease is communicable to monkeys, that it is transmitted by an insect, *Pediculus vestimenti*, and finally that it is probably caused by a bacillus which they succeeded in isolating from the blood of typhus patients and from the insects.

The volume appears to us noteworthy in several aspects. The scientific value of its contents, dealing with pioneer research in three important fields and practically covering the entire scope of essential knowledge in two of them, renders the work one of high scientific distinction and fully justifies its existence. The committee may be congratulated in perceiving what a rare opportunity existed of perpetuating the memory of a brief career by the simple record of its own activities.

These collected studies stand as a model of orderly and effective research guided by a keen imagination and scientific enthusiasm. The

volume is a unique testimonial to the genius and energy of one of the most productive of American pathologists.

J. E.

The Geology of Building Stones. By J. ALLEN HOWE. London, Edward Arnold. 1910. Small octavo, pp. viii + 455.

This work, as stated in the editor's preface, is the fourth volume of a series of works treating of economic geology, the compilation being made mainly with a view to the requirements of students of architecture.

The volume contains, in a condensed form, a large amount of information gathered from sources easily recognizable, though foot-notes are lacking and credits given mainly for trifling statements of fact rather than ideas.

The work begins with an introductory chapter which includes a table of strata arranged after the English system. This is followed in order by chapters on the minerals of building stones; igneous rocks; sandstones and grits; limestones (including marble); slates and other fissile rocks. Pages 333-411 inclusive are devoted to discussions of the decay and the testing of building stones. In the reviewer's opinion too much stress is laid upon the latter subject and too little upon the first. No amount of testing by methods now known can compare in value to a study of the conduct of the stone in the quarry bed or in old buildings. Incidentally the statement on page 398, that the present writer made certain corrosion tests, is an error. Credit should be given to Professor J. A. Dodge, of Minneapolis, Minn.¹

Naturally the descriptive portion of the work is devoted largely to English materials, but American and other foreign localities are not wholly overlooked.

As might perhaps be anticipated from the title, the various classes of sedimentary rocks are discussed with reference to their geological horizons. How far such an arrangement of the subject is desirable has always been a question in the reviewer's mind. Unless it

¹ See "Stones for Building and Decoration," third edition, p. 458.

can be shown that stone of the various horizons possess characteristics of their own it would seem that the question of position in the geological scale was wholly of minor importance. Kind, quality and accessibility are the only questions in which the man of affairs is interested or need concern himself.

In the appendices is given a list of the principal quarries, together with a bibliography, the latter confessedly incomplete and containing no reference to the important reports published in America by the geological surveys of Georgia, Maryland, Missouri, New York and North Carolina.

The work represents a laudable attempt to make certain information available to students of architecture. Whether successful or not the future must decide. At present the average architect seemingly contents himself with the purely decorative feature regardless of climate and incidental or consequential durability. Witness the proposed construction of one of the most elaborate ecclesiastical structures in America from one of the cheapest and least durable of natural materials. And this for no other reason than that the elaborate detail of ornamentation, the effect of light and shade, can not be produced in a better stone at what is considered a reasonable outlay of time and money!

GEO. P. MERRILL

Crystallography and Practical Crystal Measurement. By A. E. H. TUTTON, D.Sc., M.A. (Oxon), F.R.S., A.R.C., Vice-president of the Mineralogical Society; Member of the Councils of the Chemical Society, and the British Society for the Advancement of Science. New York, The Macmillan Company; London, Macmillan & Company, Limited. 1911. 8vo. Pp. xiv + 946, 720 figures in the text. \$8.50.

This work aims to present a complete survey of the science of crystallography from the most modern point of view, including both the theory and practise of the study of crystals and their manifold properties. Avoiding the forbiddingly mathematical treatment of his English predecessors in the field the author

has succeeded admirably in giving a living interest to crystallography such as is to be found elsewhere, if at all, only in von Groth's "Physikalische Krystallographie." The method of presentation differs however widely from von Groth's in that theoretical considerations generally follow on detailed descriptions of actual crystallographic investigations drawn from the author's wide experience. These practical details occupy a large part of this large volume and in many respects are its most distinctive and valuable feature. Tutton's work has been remarkable for the careful attention to detail which has rendered his results extraordinarily accurate; and for the completeness of his studies, made chiefly on artificial crystals. So that in the detailed records of measurement and the full description of structure and use of instruments and methods employed we have the best guide-book to actual crystallographic practise which has yet appeared. Concerning the actual measurement of crystal angles little that is new is claimed for the book; and indeed it is much to be regretted that the author treats so slightly the use of the two-circle goniometer. But the descriptions of methods in density, optical, thermal and elasticity investigations form a most welcome contribution to the scanty literature in this domain of peculiar difficulty, and the author speaks here with the authority of an undoubted leader.

The chapters in which are traced out the historical development of the theory of homogeneous crystal structure are particularly well done and are of the greatest interest. The idea of molecular distance ratios is also fully worked out and its application abundantly illustrated.

The illustrations of the book are abundant and good; the crystal drawings almost all new, the figures of instruments very clear wood-engravings and the interference figures reproductions of the author's photographs.

In all respects the work is to be regarded as of unusually high excellence and of the first importance in the field of crystallography.

CHARLES PALACHE

HARVARD UNIVERSITY.

The Optical Properties of Crystals with a general introduction to their physical properties, being selected parts of the Physical Crystallography. By P. GROTH, Professor of Mineralogy and Crystallography in the University of Munich. Translated (with the author's permission) from the fourth revised and augmented German edition by B. H. JACKSON, M.E., M.A., of the University of Colorado. 8vo, xiv + 309 pages, with 121 figures in the text and two colored plates. Cloth, \$3.50. New York, John Wiley & Sons; London, Chapman & Hall, Limited. 1910.

This is a partial translation of the well-known work of Professor von Groth, "Physikalische Krystallographie," which is generally regarded by those who have to deal with optical crystallography as the best non-mathematical treatise on this subject yet produced. The translation, to quote from the translator's prefatory note, "is made up chiefly of matter contained in Part I. of the original work, on the properties of crystals; besides embracing the general introduction and all that falls under the heading optical properties in this part, it includes also whatever may be found there on the influence of other properties on the optical properties. Short extracts from Parts II. (Systematic Description of Crystals) and III. (The Methods of Crystal Investigation) have been introduced, on occasion, for illustration and example."

The scope of the work may be gathered from the headings of the principal divisions which are as follows: General Introduction to the Properties of Crystals; The Nature of Light; Combination (Interference) of Plane-polarized Light; Optically Isotropic Bodies; Double Refraction of Light; Optically Uniaxial Crystals; Optically Biaxial Crystals; Recapitulation: Classification of Crystals According to their Optical Properties; Combinations of Doubly Refracting Crystals to show the Character of their Double Refraction; Rotation of the Plane of Polarization of Light in Crystals; Absorption of

Light in Crystals; Influence of other Properties on the Optical Properties of Crystals including Thermal Properties, Elastic Strain by Mechanical Forces and by Electrical Forces, Permanent Strain, and Twinning.

The translation is excellent, the English being free and idiomatic but following closely the original text. The work is entirely within the comprehension of any student who knows the rudiments of crystallography and forms a much-needed and very welcome addition to the English text-books in the field covered by it.

The colored plates reproduced from the original work are excellent; I comprises a spectrum of white light and a Newtonian color scale of the first four orders; II presents the important types of interference figures in convergent light in thirteen figures.

An appendix contains a useful list of German and American supply houses for apparatus, models, crystals and preparations. English firms might well have been added to this list.

CHARLES PALACHE

SPECIAL ARTICLES

WEST ELIZABETH, PENNSYLVANIA, DEEP WELL¹

I AM indebted to Dr. I. C. White,² state geologist of West Virginia, for calling my attention to the omission from my paper published in SCIENCE, May 26, 1911, under the title "Underground Temperatures," of an important deep boring made in 1897 in Allegheny County, Pa. The data relating to this well are so important as to be worthy of a separate note.

The well is located on Peter's Creek about two and one half miles west of West Elizabeth, Allegheny County, Pa., and about twelve miles south-southeast of Pittsburgh. It is the deepest well drilled in the United States

¹ White, I. C., West Virginia Geological Survey, Vol. I(A), 1904, pp. 103-107. Hallock, W., "Subterranean Temperatures at Wheeling, W. Va., and Pittsburg, Pa.," *School of Mines Quarterly*, 1897, Vol. XVIII., pp. 148-153; see especially pp. 151-153.

² Personal communication, June 6, 1911.

and was put down by the Forest Oil Company in 1897. The well was dedicated to science and had for its purpose drilling down and into the Corniferous limestone, but after a depth of 5,575 feet was reached an accident beyond repair occurred and further drilling was from necessity abandoned. The well was begun 130 feet below the Pittsburgh coal, and after passing through rocks of the Carboniferous (Pennsylvanian and Mississippian) and of the Upper and most of the Middle Devonian, was bottomed (5,575 feet) in supposed Marcellus black shale, probably not more than 100 feet above the Corniferous limestone. The vast thickness of rocks penetrated by the well were all sedimentaries, including, according to the log,³ shales, slates, coal, sandstones and limestones, as the chief lithologic types.

At the request of Dr. White, Professor William Hallock, of Columbia University, was afforded every facility for measuring the temperature of the well. A brief statement of the temperatures measured in the well was published by Professor Hallock in 1897.⁴ Five measurements made at different depths are recorded by Dr. White. These may be tabulated as follows:⁵

TEMPERATURE MEASUREMENTS IN WEST ELIZABETH DEEP WELL

Temperature at		Difference in temperature for	Kind of rock
525 ft.	57° F.		Sand.
2,252 ft.	64	1,677 ft. 7° F.	Slate
2,397 ft.	78	445 ft. 14	Slate and shells.
5,010 ft.	120	2,613 ft. 42	Limestone.
5,380 ft.	127	370 ft. 7	Slate.

The figures in the table above explain themselves and need no comment except that the increment of heat is shown to be exceedingly variable, and is in accord with many other deep wells over the earth's surface in which temperature measurements have been

³A complete log of the well is published by Dr. White in Volume I(A) of the West Virginia Geological Survey, 1904, pp. 104-107.

⁴Hallock, W., *School of Mines Quarterly*, 1897, pp. 151-153.

⁵Data taken from Vol. I(A) of West Virginia Geological Survey, 1904, pp. 104-107.

made. The explanation offered for the variation in temperature shown in this well is the presence of a considerable flow of natural gas from the Bayard sand at 2,282-7 feet.⁶ The average increment of heat for a depth of 4,855 feet, which represents the difference between the least (525 feet) and the greatest (5,380 feet) depths at which temperature measurements were made, is 1° F. for every 69.37 feet.

THOMAS L. WATSON

UNIVERSITY OF VIRGINIA

ADDITIONAL NOTE ON RETICULATED FISH-SCALES

SINCE the publication of my recent account of dipnoan fish scales in *SCIENCE*, some interesting facts have come to light.

1. Dr. L. Hussakof, of the American Museum of Natural History, has very kindly placed in my hands scales of *Sagenodus* from the Carboniferous rocks of Mazon Creek, Illinois. A well-developed scale is oval, about 50 mm. long and 37 broad, and in appearance and structure essentially agrees with the scale of the living (Australian) *Neoceratodus*. The reticulations are evident, and the very fine basal longitudinal fibrillæ are minutely tuberculate. Thus we have positive evidence of the enormous antiquity of this type of scale, including even the details of structure.

2. A specimen of the sucker *Moxostoma cervinum* Cope, collected by Dr. B. W. Evermann, proves to have two kinds of scales. One has a quadrate form, with strong laterobasal angles, strong apical and basal radii, the circuli dense in the basal and lateral fields, but widely spaced in the apical. This is the sort of scale we are accustomed to find in *Moxostoma*, a scale strongly suggestive of various old-world cyprinids. The other type of scale has the laterobasal angles more rounded, radial lines running to the margin

⁶Professor Hallock states that "the thermometers at 2,250 feet indicated a cooling due to the expansion of the gas amounting to about 14°." *Op. cit.*, p. 153. Gas, volume 25 lbs. per min., West Virginia Geol. Survey, Vol. I(A), p. 105.

⁷Professor Hallock gives the increment of heat from top to bottom (5,000 feet) of well as 1° F. for 71.5 feet. *Op. cit.*, p. 150, table II.

all around, and the very broad central area occupied by irregular, more or less elongated reticulations. Thus the scale comes to closely resemble those of the Mormyridæ. As it now seems evident that the ancestors of the Teleosteans must have had reticulated scales, or at least that the ordinary radial sculpture is derived from the reticulated type, this *Moxostoma* scale must be regarded as uniquely primitive or atavistic for the general group to which it belongs.

3. Dr. G. A. Boulenger has very kindly sent me scales of the cæciliid amphibian *Ichthyophis glutinosus*. These are very small, embedded in the skin, cycloid in form. The pattern is extremely characteristic, consisting of concentric grooves connected at intervals by cross-lines, the whole effect being like that of bricks in a wall. The concentric grooves are probably not circuli, nor can I make out anything corresponding to the circuli of fishes. In parts of the scales, however, the markings become irregular, producing a reticulation which closely simulates that of the reticulate-scaled fishes. I believe that the scales are really comparable to fish-scales, and that the sculpture is the same as the radial sculpture of fishes. No fish scale has been seen resembling in detail that of *Ichthyophis*; such scales as those of *Chrosomus* are superficially similar, but owe their circular lines to different elements.¹

T. D. A. COCKERELL

UNIVERSITY OF COLORADO

NOTES ON THE GENUS *TYPHA* AND ITS NEMATODE
ROOT GALL—*HETERODERA RADICICOLA*
(GREEFF) MULL.

DURING the summer of 1908, while investigating some problems connected with the root system of *Typha latifolia*, I found a number of abnormal growths on the rootlets. These growths appeared as irregularly spherical or fusiform enlargements, varying in size from 1 to 5 mm. in diameter. They were identified by Professor Atkinson as root galls caused

¹ Since this was written, I have found that a deep-sea eel, *Synaphobranchus pinnatus*, has scales curiously similar to those of *Ichthyophis*.

by the nematode *Heterodera radicicola*. I have collected these galls at the same station (limnology station of Cornell University) three successive years, but have never found them on *Typha* in any other locality.

Professor Atkinson¹ thought, from his observations of this worm on potatoes and tomatoes, that, if favorable opportunity should occur for its introduction in the north, it might become a pest. Webber and Orton² say it will never become a serious pest in the north, as severe cold kills the worm. Van Hook³ reports the worm as wintering in ginseng beds which had been mulched and also in protected forest beds. This worm has been a serious pest to ginseng in the north.

Stone and Smith⁴ found the galls on outdoor plants, but concluded that they were transient.

The plants observed by me in the Cayuga marshes are located along the shore line of one of the arms of Fall Creek where moisture is plentiful in the soil all winter. Winter observations prove that the soil in which the galls are found does not freeze. None of the galls have been found more than eighteen inches below the surface.

L. N. HAWKINS

CORRELATION NOTES

In describing the fauna of the Moorefield shales of Arkansas¹ Mr. George H. Girty lists and describes the following fossils among others from the region: *Productus inflatus* var. *coloradoensis* Girty (?),² *Productus arkansanus* var. *multiliratus* Girty,³ and *Diaphragmus elegans* Norwood and Pratten.⁴ By a comparison of the figures of these fossils on plate iv.⁵ with fossils which the writer collected

¹ Bull. 9, Alabama Exp. Sta.

² U. S. Dept. Agr., Bur. Plant Ind., Bull. 17, 1902.

³ Cornell Agr. Exp. Sta., Bull. 219, 1904.

⁴ Bull. 55, Mass. Agr. Exp. Sta., 1898.

⁵ "The Fauna of the Moorefield Shale of Arkansas," U. S. Geol. Survey, Bulletin No. 439.

² *Ibid.*, pp. 42-43.

³ *Ibid.*, p. 43.

⁴ *Ibid.*, pp. 51-52.

⁵ *Ibid.*, plate iv.

at Fort Apache, Ariz., in 1902, he finds that the three fossils mentioned are abundantly represented in the lower Red Wall there, especially in the limestone series that caps the formation on the mesas east of the North Fork of White River.* Specimens of these species, collected from this region then, are to be found in the writer's collection in the Geological Museum of the University of Indiana. The finding of the similar fossils in the two districts would seem to indicate that the strata concerned are relatively of the same age.

ALBERT B. REAGAN

NETT LAKE, MINN.

SOCIETIES AND ACADEMIES

THE TORREY BOTANICAL CLUB

THE meeting of April 11, 1911, was held at the American Museum of Natural History at 8:15 P.M. Dr. E. B. Southwick presided.

The regular order of business was dispensed with and the announced lecture of the evening on "Poisonous Mushrooms," by Dr. W. A. Murrill, was then presented. The lecture was illustrated with many lantern slides. An abstract of the lecture prepared by the speaker follows. A more complete discussion of the subject by Dr. Murrill may be found in the November number of *Mycologia* for 1910.

"Considering its importance, it is remarkable how little is really known about this subject, most of the literature centering about two species, *Amanita muscaria* and *Amanita phalloides*, which have been the chief causes of death from mushroom eating the world over.

"As the use of mushrooms in this country for food becomes more general, the practical importance of this subject will be vastly increased, and it may be possible to discover perfect antidotes or methods of treatment which will largely overcome the effects of deadly species. This would be a great boon even at the present time, and there will always be children and ignorant persons to rescue from the results of their mistakes. Another very interesting field, both theoretical and practical in its scope, is the use of these poisons in minute quantities as medicines, as has been done with so

*Reagan, Albert B., "Geology of the Fort Apache Region in Arizona," *American Geologist*, Vol. XXXII., pp. 265-308.

many of the substances extracted from poisonous species of flowering plants, and even from the rattlesnakes and other animals. Thus far, only one of them, the alkaloid muscarine, has been so used.

"The poisons found in flowering plants belong chiefly to two classes of substances, known as alkaloids and glucosides. The former are rather stable and well-known bases, such as aconitine from aconite, atropine from belladonna, nicotine from tobacco and morphine from the poppy plant. Glucosides, on the other hand, are sugar derivatives of complex, unstable, and often unknown composition, such as the active poisons in digitalis, hellebore, wistaria and several other plants.

"The more important poisons of mushrooms also belong to two similar classes, one represented by the alkaloid muscarine, so evident in *Amanita muscaria*, and the other by the deadly principle in *Amanita phalloides*, which is known mainly through its effects. Besides these, there are various minor poisons, usually manifesting themselves to the taste or smell, that cause local irritation and more or less derangement of the system, depending upon the health and peculiarities of the individual.

"The principal species of poisonous fungi were illustrated by colored lantern slides, the series containing *Amanita cothurnata* Atk., *Amanita muscaria* L., *Amanita phalloides* Fries, *Amanita strobiliformis* Vittad., *Clitocybe illudens* Schw., *Inocybe infide* Peck, *Panus stypticus* Fries, *Russ emetica* Fries, and several other poisonous species of interest."

THE meeting of April 26, 1911, was held in the museum building of the New York Botanical Garden at 3:30 P.M. Vice-president Barnhart presided.

The first number on the announced scientific program was a paper, on "Fern Collecting in Cuba," by Mrs. N. L. Britton. This paper is published in full in the *American Fern Journal*, Vol. I., p. 75.

The next number was a discussion of "Fern Venation," by Miss Margaret Slossen. A more complete discussion of the subject by Miss Slossen may be found in her book "How Ferns Grow."

The meeting then adjourned to the Fern House of the New York Botanical Garden under the guidance of Mrs. N. L. Britton for a further study of ferns.

B. O. DODGE,
Secretary